

Price (90s.)

GONORRHOÆAL DISEASES
OF THE
UTERINE APPENDAGES.



GONORRHOEAL DISEASES OF THE UTERINE APPENDAGES.

By JOSEPH PRICE, M.D.

[Read February 13, 1889.]

THE attitude of numbers of professional men who express either incredulity or absolute disbelief in the causative relation between gonorrhoeal disease in women and pyosalpinx and abscess of the ovary, is sufficient justification for a still further discussion of this subject. My views upon the matter are based neither upon theory nor upon microscopic examination. They are from surgical experience only or from confessions of men whose wives have been diseased by them. From the time that Noeggerath first formulized his belief upon this subject it has been smiled at, contradicted, or controverted, but never in its essentials disproven. In his earlier paper Noeggerath fell into the common error of enthusiasts, that of attributing too much to his discovery, and claiming too wide a pathological field as the sequelæ of this trouble. This, without doubt, led many otherwise fair-minded men to pass over his paper as unworthy of attention, thus impeding the progress that otherwise would have followed its discussion and the observations based upon its claims. In taking up most of the later surgical works we find the etiology of ovarian and tubal disease considered from this standpoint omitted—a missing link, or differentiated out of sight. This is wrong. As early as 1877 Mr. Lawson Tait and others insisted upon the relation existing between gonorrhoea in man and tubal disease in women. Noeggerath antedated him about five years. Mr. Tait also insisted on its causative relation to perimetritis, this as late as 1883. Schroeder, in the early editions of his *Gynecology*, insisted upon this as bearing a causative relation to ovarian and tubal troubles. In the very latest edition he says: "Gonorrhoea, in the highest degree, appears as a causative disease in women." Säger also is an ardent advocate of the same belief. He is wrong, however, I am persuaded, in holding that the gonorrhoeal infection is always late in revealing

its presence in the woman when transmitted by the man. To this subject I shall refer later.

Without further collation of authorities upon this subject, I shall proceed briefly to its discussion. Whether or not the presence of the disease can be diagnosticated absolutely by the presence of the gonococcus of Neisser, is of small importance, if by the chain of common evidence we can connect the presence of one disease with the other in their sequence. If, on discovering tubal disease in a woman who has never aborted nor had any of the diseases incident to childbed, who has been healthy up to a time, after which vaginitis has occurred, contracted from her husband, after which the woman from time to time experiences increasing pelvic pain, losing strength and weight—the case, it seems to me is made out, save as quibbling may dispute it. This history occurs in most of the cases I have handled. Of the many cases that have come under my observation, I choose the following as illustrative and typical:

A young married woman, one child. Her recovery from childbed excellent; no gonorrhœal infection of the child at birth. Some months afterward she had inflammation of the vulvo-vaginal glands, with suppuration. Later she appeared with abdomen tense and painful, enlarged tubes and ovaries, tender and painful on the slightest movement or pressure; she had lost in weight and strength. Her husband confessed to the infection of his wife. The diagnosis was made of gonorrhœal pyosalpinx, and operation proved the correctness of the opinion. Both tubes contained pus, were cheesy and friable—the ligatures cutting through all but the vessels. The abdomen was full of fluid, and the intestines gave evidence of acute peritonitis.

The history here is complete, leaving no possible doubt as to the origin of the disease. The early infection here exhibited is at variance with the views of Sânger and shows that his statements are not necessarily correct, or are but accidentally correct, if at all so. There is no sufficient reason why this infection should not be early. I incline to the belief that the disease originates early, but may be slow in its progress, and thus escape attention and discovery.

DISCUSSION.

DR. J. WILLIAM WHITE: I had not intended to take part in the discussion, but as you have kindly asked me, Mr. President, I will say that neither the paper read this evening, nor anything else that I have heard or read, has convinced me of the frequent relation between tubal disease in the female and gonorrhœa in the male. I believe that such cases as Dr. Price details are

defective in some important point. They assert that the occurrence of suppurative disease of the tubes is a consequence of preëxisting disease in the husband, or of the individual with whom the woman has had connection. It seems to me that by a similar process we could demonstrate the relation between gonorrhœa in the male and mitral disease, cataract, or other ailment in the woman. This is another instance of the *post hoc ergo propter hoc* line of argument. It is carrying a theory, which has an undoubted basis in fact, to great extremes, and, as regards the views of Noeggerath, to ridiculous extremes. His sweeping assertions in regard to the persistence of latent gonorrhœa in the male, and his theory that women having connection with men who had ever had gonorrhœa, are constantly rendered barren, or if they do conceive do so only to abort, and that such women are the constant subjects of ovarian and tubal disease, are, I believe, contradicted by the practical experience of every surgeon and of every general practitioner.

I have had rather exceptional opportunities for examining cases of gonorrhœa in the male. Many of these cases have married, and the majority of them had, and still have, healthy wives and healthy children. While there is undoubtedly some foundation of truth in regard to this matter, and while there are undoubted instances of this form of infection, and while gonorrhœa in the male may produce vaginitis, cervicitis, endocervicitis, and pyosalpinx, yet it seems to me that these occurrences are exceptional, by no means so frequent as is claimed, and certainly not the rule. Dr. Price does not assert his belief in these views, but I am surprised at the favorable tone of his comments upon them. I have always regarded the theories of Noeggerath as examples of the extremities to which an enthusiast may be led, and only to be fitly characterized by the somewhat strong terms of absurd and ridiculous. Certainly in my personal experience I have never seen anything to warrant a belief in the relation of these two conditions, as cause and effect, in any very large proportion of cases.

The pathological side of the subject, to which Dr. Price merely alludes, is, after all, the one of greatest importance. If gonorrhœa in the male is, as Noeggerath describes it, latent in a majority of cases, it must be that it depends on a peculiar microörganism, and the invariable association of the gonococcus, or some other low form of animal life, with gonorrhœa, must be demonstrated. In this "latent" gonorrhœa, described by him, there may be no symptoms, and nothing which would enable the practical specialist in venereal diseases to recognise the presence of the affection. There must then be demonstrated at least that some microbe is present which belongs to gonorrhœa and produces the disease whenever it develops in the urethra. In order to demonstrate the relation between latent gonorrhœa and these diseases of the tubes and ovaries, some such microörganism should also be found in these latter cases. I believe that in certain persons the nidus for the multiplication of the organism is exceptionally favorable. In such cases it rapidly multiplies and tends to spread, and increases the severity of the disease. These are the cases, I doubt not, in which tubal disease follows infection. That it is frequent, or that there is demonstrated any invariable relation between the existence of the microörganism and such diseases in the female, I do not believe has been proven. The clinical relation of the two conditions being, therefore, to my mind at least, not established; and the pathological evidence being en-

tirely wanting, I am compelled to reject as unproven and unfounded the theories which so closely associate an antecedent gonorrhœa in the male with so many forms of disease of the uterine appendages. I believe those theories to have been harmful in their influence upon gynæcological practice in having given apparent justification to a large number of operations, a fair proportion of which I do not doubt will be shown within the next ten years to have been unnecessary and injudicious.

DR. WILLIAM S. STEWART: I am much interested in this subject, which is so much agitated at the present day. I am interested in the fact that so many tubes and ovaries are being removed. It seems to me an alarming thing that at the present time this should be the prevailing disease of women. I think, therefore, that it is a matter of great importance that we should come to some pathological determination as to the nature of the contagion of this disease, and whether its prevalence is due entirely to gonorrhœal infection, or to other causes.

I have had some little experience recently in exploring the tubes. I was anxious that some of the specialists who make removal of the tubes and ovaries an every-day matter, should have witnessed the treatment of a case. I succeeded in passing an aluminium applicator through the diseased tube, and in permitting the escape of the pus which the tube contained, and the patient, who I feared was going to die, recovered. The diminution of the enlarged tube was a matter of great satisfaction to me, as I feared that my previous treatment of the case had been the cause of the development of the disease.

I have passed the probe into the tubes in two other instances, and the results which followed—diminution of the inflammation and liberating the pus—has been a matter of great gratification as well as surprise.

DR. HOFFMANN: Dr. White's remarks show how two persons interested in the same subject may arrive at entirely opposite conclusions. Dr. White has had abundant experience in dealing with these cases, especially in the almshouse, in a class of people where he would be supposed to see the relation between gonorrhœa in the male and certain diseases of the female, especially in the uterine appendages. He, however, fails to connect the two in any marked degree. Bernutz has written a very remarkable work, appearing in the New Sydenham publications a few years ago, and the conclusions which he reaches are diametrically opposed to those of Dr. White. Bernutz in examining 99 cases of pelvic peritonitis in women, concluded that 28 were distinctly attributable to gonorrhœa; 43 were traced to childbirth, and 8 of the childbirths were abortions. If you take away the 43 cases, due probably to infection from some incidental cause, we have remaining 56 cases, of which 28, or 50 per cent., were traced not only theoretically, but by a post-mortem examination, to gonorrhœa, all other causes being eliminated. Taking his statistics of the 28 cases which he considered analogous to orchitis in the male, we have 20 menstrual and 8 traumatic. The causes of traumatism were, in 3, venereal excesses; 2, syphilis of the cervix; 2, the introduction of the sound; and 1, the use of the cold douche during menstruation.

I have myself seen this condition so frequently that I cannot regard it as an unusual accident, and all the less after reading the remarkable record of which I have here a note, can I think that it is accidental. So far as Noeg-

gerath's views are concerned, I think that Dr. Price sounded their value. His views are extreme, and we may as well now as ever regard them as extreme. There is no use in laying down the principle that only one cause can produce the disease. The original paper of Bernutz, published in 1857, antedating the publication of Noeggerath, is to me convincing.

Whether or not we always find the gonococcus of Neisser is to me a question of indifference, and not always decisive. We know very well that one microbe will, by its presence, crowd out another. When we open the abdomen and find the tubes filled with stinking pus, in which putrefactive changes have taken place, I am not surprised that the gonococcus is not found, its place being taken by some other form of micrococcus. In such cases I do not believe that the gonococcus is diagnostic.

DR. WHITE: The statistics of Bernutz and Goupil, to which reference has just been made, are quite familiar to me, although it was my impression that they were published in 1861, not 1857. This is quite a different question from the one I was discussing. These were prostitutes of the lowest class undoubtedly infected with gonorrhœa. The proportion of these cases in which pelvic peritonitis occurs is considerable. This is entirely distinct from the cases in which you depend upon the history of a previous and often a long distant gonorrhœa in the husband, to explain ovarian and tubal disease in the woman, but in which you have no consecutive history of true gonorrhœal infection of the female. It is to this that I object, not to the assertion that pelvic peritonitis and pyosalpinx *may* occur in some cases of gonorrhœa, particularly in women of the lower classes. Dr. Hoffmann has missed the point of my remarks. I have quoted the statistics of Bernutz in a paper which has been accepted by the *British Medical Journal*, although not yet published. They simply present the records of two of the French hospitals to which the lowest class of French prostitutes are admitted, women broken down in health, and the subjects of virulent forms of gonorrhœa. This is entirely distinct from the proposition of Noeggerath, which connects not only existing disease in the male, but also precedent disease of which all active symptoms may have disappeared, with certain conditions of the female—barrenness, tendency to abortion, and disease of the uterus and appendages. It is to that I object, and not to the assertion that in a large proportion of cases these complications occur in prostitutes with well-marked gonorrhœal infection.

DR. M. PRICE: In regard to the statement of Dr. White that these diseases are more peculiarly connected with these hospitals and these women of low character, I would say that all the cases that I have had in private practice, six or eight in number, have been not only of the better class, but among the best women of this town. The specimens from one of these cases I should like to show to the Society. This pair of tubes is unquestionably gonorrhœal. I treated the man some ten years ago. I afterwards treated his wife for old-fashioned cellulitis and peritonitis. She has had five attacks during the last eight years, in every one of which there was high temperature and quick pulse, with the pelvis filled with inflammatory masses. These inflammatory conditions were cured about as the electrician cures pelvic abscess and pyosalpinx. The health and vigor of the woman were sufficient to cause absorption of the liquid portion of the pus, and for a time the disease was kept in

abeyance. Four or five days ago I removed from this woman's pelvis a pint of pus of the most offensive character. This was her last struggle, her last effort to get rid of it. She was unequal to the task, and was slowly dying of a septic condition. The symptoms were of such an urgent character, that I opened the abdomen and removed both tubes. There we have a marked case of pyosalpinx unquestionably of gonorrhœal origin. Out of the eight or nine cases of pelvic trouble I have had occasion to operate on, in all but one I could distinctly trace a gonorrhœal origin. In every one I had treated the man for gonorrhœa. From that day not one of these women bore a child. They always complained of symptoms of pelvic trouble, pain on pressure, pain on movement, and pain on slipping or jarring. The trouble in these cases was just as certainly gonorrhœal in origin as that any trouble existed.

The cases that Dr. Hoffmann has referred to as occurring after labor, have, I believe, in many instances a positive gonorrhœal origin. I recall one case of abortion where there was for a year previous unquestionable evidences of tubal trouble on one side. The abortion was followed by acute tubal inflammation, sepsis, and death. An attempt was made to relieve her, but it was too late. My impression is that a perfectly healthy woman, who has never been inoculated with gonorrhœal poison, is very slow to take on pelvic inflammatory trouble. It probably does exist, but there is usually a poison which has produced the condition, which these other incidental accidents aid on and help.

DR. WHITE: I should like to ask Dr. Price what symptoms of gonorrhœa these women presented in addition to those which he has mentioned. To my mind, the argument, as I said before, seems to be entirely of the *post hoc ergo propter hoc* style, and it would apply just as well to a series of cases of goitre. It would not be difficult in a large number of these cases occurring in the best society, to demonstrate that ten years previously their husbands had gonorrhœa, but it is evident that in both series of cases there would be a missing link in the chain of evidence.

DR. M. PRICE: A man comes to me stating that he is in trouble, and that he fears that his wife has been inoculated. I treat the husband for unquestioned gonorrhœa. In the course of ten days or two weeks, I treat the wife for unquestioned gonorrhœa. I think that is sufficient evidence as to the cause. I do not see any relevancy in the reference to goitre or heart disease. We know why these come in some instances. It is as plain to me as the nose on Dr. White's face that these conditions come from gonorrhœa, and from no other cause.

DR. JAMES COLLINS: I wish to ask one question. I have seen something of practice in my life, and I should like to hear something said with reference to the proportion of cases in which this trouble occurs. Taking the cases of gonorrhœa in the male, I should like to know in what proportion of cases these sequelæ occur?

DR. HOFFMANN: I do not wish to cross fire; but it seems to me that as Dr. White has suggested that I have missed the point of his remarks, I might say that he has missed the point of what I said. I do not absolutely accept Noeggerath's views, which I consider extreme. I do not believe that a man who has once had gonorrhœa and has been completely cured, will necessarily transmit the disease. Dr. Price did not advocate that view. The only

question is this: whether in the absence of any other proven cause, such as abortion, exposure to cold, the introduction of the sound, or the like, and gonorrhœal infection proven, the disease may not be caused by the gonorrhœa. I think that when that is proven by experience with low-grade prostitutes with the same anatomy and physiology as the high-born ladies, we may apply our experience with the former to the latter. The whole thing is, therefore, narrowed down to this: whether or not we are justified by the exclusion of all other causes in saying that gonorrhœa is the cause. This brings us to a rational consideration of the question, and not the extreme views suggested by Noeggerath. The trouble is that in considering these matters men are apt to go to extremes, and to say that one condition or another is the invariable cause of the disease. I have seen tubal disease come from all the causes enumerated by Bernutz. The treatment of the disease must of course modify the condition as propagated from one sex to the other.

DR. J. M. BALDY: The views of Noeggerath, Bernutz, Sinclair, and others, including the author of the paper of to-night, are very extreme, and grossly misrepresent the facts. Venereal disorders in man are not so frequently the cause of pelvic inflammatory diseases in women as these men would have us believe. I think with Dr. White, that in order to settle this question, we must have more than mere clinical evidence; we must also have pathological research and experimentation. So far as the clinical aspect of these cases is concerned, I may say that the vast majority of them coming under my observation are not women who have never been pregnant. There is almost always a history of abortion or labor, with septic trouble at that time. Sinclair has demonstrated that most of these patients have the gonococcus present in the secretions of the vagina and uterus, and for that reason he has considered the disease to be of specific origin; in some cases where the gonococcus was not found he still believed the disease to be of specific origin, supposing that the gonococcus had been crowded out by other forms of micrococci.

The question arises whether the gonococcus causes gonorrhœa. I think that it has been positively settled by the experiments of Sternberg that the gonococcus is not the cause of gonorrhœa, and that it may exist in other discharges. He made pure cultures of the gonococcus and inoculated the eyes of animals with negative results. The vaginæ of animals were inoculated with these cultures, with the same result. He then secured three hospital patients who were under observation in bed, and inoculated the urethras of these men with the pure culture of gonococcus. The results were absolutely negative. The only observations opposed to this are those of Bokard, made at Buda-Pesth. He submitted the urethras of half a dozen medical students to experimentation. At the end of a week three of the students were found to have well-marked gonorrhœa. At first sight these experiments seem as conclusive as Sternberg's; but when we recall the fact, that these patients were medical students, and had a week's liberty before and after inoculation, in a city with the moral tone of Buda-Pesth, the experiment is absolutely worthless.

Sternberg found a microorganism in urine which had undergone alkaline decomposition so like the gonococcus that the two could not be differentiated. He has also found it in the pus taken directly from a whitlow. I think that in view of these observations, we can consider that it is proven conclusively, that the gonococcus is not the cause of gonorrhœa, and therefore whether the

gonococcus is present or not, it makes little difference; its presence is not proof that the condition is due to gonorrhœa. In accepting this view we can plainly see that the conclusions drawn by Martin, Sanger, and other Germans, from the presence of gonococcus, are erroneous.

In regard to cases of chronic gonorrhœa or gleet, where, following gonorrhœa, there has been a stricture for years, with a little discharge, either mucous or pus, observed in the morning: such cases will no more cause gonorrhœa than will the gonococcus. Up to the present time no experiments have been made on this subject. I am at present engaged in investigating this matter, but am not yet ready to give the final results, as my observations are not yet completed. I will, however, say this: I have taken the discharge from the cases that I have mentioned, and inoculated the eyes of rabbits, with entirely negative results. Not satisfied with taking these discharges when the urethra was in a quiescent state, I have irritated the parts by the passage of sounds two sizes larger than the men had been accustomed to use, and the next morning inoculated the eyes of rabbits, but still with negative results. Then fearing the criticism that Sternberg did, that possibly animals were not susceptible to this infection, I secured a man and inoculated his urethra with this discharge, and so far with just as absolutely negative results. If we prove that these cases of gleet and chronic gonorrhœa will not cause gonorrhœa, that the gonococcus will not cause gonorrhœa, and that the gonococcus is found in other discharges as well, we come down simply to the acute cases of clap as the cause of these pelvic inflammatory diseases in women. This is exactly where we stand. That a man with acute gonorrhœa will inoculate a woman is beyond peradventure true, and that the inflammation so set up will travel along the mucous membrane of the vagina, uterus, and Fallopian tubes, causing pelvic inflammatory trouble, is beyond cavil. That we can prove more than this I think is impossible. Pathological research and experimental investigation are against it. There remains nothing but the clinical facts. And here again we have illustrated how two different minds will approach a subject from the same standpoint, and yet draw different conclusions. In many of the cases in which I have seen Dr. Price draw the conclusion that the trouble was due to gonorrhœa, I could find no proof of that fact to satisfy my own mind. To me the history of gonorrhœa was most eminently unsatisfactory, while the history of puerperal septic infection, which he chose to ignore altogether, was perfectly plain and clear. I think that in many of these cases of pelvic inflammatory trouble, the origin is of a septic character, puerperal, or from dirty instruments. I should say that three-fifths of the cases that have come under my observation, were of septic origin, and that one-fifth was of gonorrhœal origin, leaving one-fifth for all the other causes.

DR. GEORGE STRAWBRIDGE: Are we to understand that the inoculation of mucous membranes with gonorrhœal pus is negative?

DR. BALDY: No, sir. I refer to the discharge from gleet, where the discharge has been present for years. Such discharges produce no effect when inoculated.

DR. STRAWBRIDGE: Some of the worst cases of conjunctivitis that I have seen have been due to the inoculation from chronic gleet. In some of these cases the entire eyeball has been destroyed in the course of forty-eight hours. In these cases the acute attack had passed over one or two months before. I

have seen dozens of such cases in my own practice. I am quite amazed that the gentleman could use this pus without trouble following.

DR. WHITE: I think that Dr. Baldy and Dr. Strawbridge are talking about different conditions. I have never seen gonorrhœal conjunctivitis from gleet. The cases seen by Dr. Strawbridge have been in the later stages of acute gonorrhœa. He speaks of one or two months. Gonorrhœa will often retain its infective properties for that length of time. Gleets, depending as they commonly do, on strictures of large calibre, and on catarrhal conditions of the mucous membrane, are attended by discharges largely mucoid in character, only under exceptional circumstances mucopurulent, and are not contagious except in rare instances. A creamy or milky discharge is apt to be infectious, while a mucoid discharge is, as a rule, innocuous.

DR. ARTHUR V. MEIGS: I wish to say a few words bearing upon the purely pathological side of the question. As yet I have only made a thorough examination of two specimens, and these were given me by Dr. Penrose. In neither was there what could properly be termed pyosalpinx. In one there was no trace of pus, but extensive inflammation and thickening of both tube and ovary, with adhesion of the tube to the ovary. In the other specimen there was a pocket of pus extending from the fimbriated extremity of the tube, its other attachment having been broken in the removal. There was, however, no pus within the tube, but as in the other case, thickening, inflammation, and adhesions. I think that it is still an open question whether or not nature could remove such a degree of inflammation as was present in the first specimen, though there is every reason to believe that nothing but operation could have removed the adhesions.

DR. J. PRICE: I am not surprised that Dr. Strawbridge is amazed at Dr. Baldy's remarks. At the present time we are in possession of sufficient clinical evidence to prove what Dr. Strawbridge has said in regard to the causal relation between gonorrhœa and gonorrhœal ophthalmia. Years ago Dr. Agnew said that he dreaded to see a case of chronic gleet of two weeks' or two years' standing come to his office; and it is just these cases that can destroy the eye. The records of eye hospitals abundantly prove this.

We now care nothing, or next to nothing, about what Noeggerath wrote, for we possess sufficient clinical material in our own surgical experience to prove that gonorrhœa, and nothing but gonorrhœa, is responsible, except in rare instances, for the pathological conditions with which we surgically daily deal. Dr. Deaver and Dr. White have proven this in their own daily experience by sections for pus tubes due to gonorrhœa.

Dr. Baldy has alluded to my cases, but he heard only the history of the patient in front of him; but in every case, I subsequently obtained the history of gonorrhœa and stricture from the husband. The question of the influence of gonorrhœa in the production of pelvic diseases in women is one not necessarily dogmatic nor mathematical, but involving probabilities. As to the influence of this disease in producing a serious train of sequelæ in the male, there seems to be no dispute; why there should be such a disparity between cause and effect, with only sex to influence the condition, is a question that I do not believe the sceptics will attempt to explain. I have certainly no explanation for gratuitous disposition.

In the class of patients met at the almshouse, among the males there is no

question as to the great prevalence of gonorrhœa. Among the females it exists about as often. In his description of a case, in the *Medical and Surgical Reporter*, December 15, 1888, Dr. White says: "Two small patches of ulceration existed on the floor of the membranous urethra, each measuring about two lines in diameter." This case may be taken as a type of the many cases of which no microscopical examination can be made, or at least is made. Further on in this same paper, a troublesome case of gleet is noticed, having existed for several years. That the presence of gleet is a real source of infection is no longer doubted. Why then dispute that an existing cause of trouble must stop short and lose its virulence as soon as it touches the female organs? Among the complications of gonorrhœa in the male, noted and admitted by good authorities, are peritonitis, subperitoneal abscess, perinephritic abscess, together with the only too frequent lesions of the bladder, prostate, and kidneys. Now, if peritonitis can occur with gonorrhœal origin through the roundabout road of the vasæ deferentes and seminal vesicles, how much more likely is it to occur in women by the direct route from the vagina, through the uterus and Fallopian tubes to the peritoneal cavity? Sinclair, in his little book, faulty and full of shortcomings as it is, proves without doubt that most of the cases of pelvic peritonitis attributed by him to gonorrhœa are rightly so ascribed. Out of fourteen cases cited, in three the diagnosis is not proven and may be regarded as doubtful. It is the danger of every enthusiast to see too much in his theory. This was the cause of the slow reception of the theory advanced by Noeggerath. Noeggerath's view is not a new one; it is only the wide application that causes it to differ from opinions held long before. I can readily understand why the profession should be practically ignorant of the ravages of this disease. Nothing short of a rigid special training will enlighten the profession in this field. Again, it is additionally strange that in this age of boasted progress the profession should question the great prevalence of gonorrhœa in the female, knowing it to be so very common in the male.

Many important investigations have been made by Bumm, Säger, and Oppenheimer. Donné, in 1844, discovered a parasite in the urethral discharges which he named *trichomonas vaginalis*. It is folly here to allude to the numerous small beasts that have been found in genital discharges—fore-runners of Neisser's gonococcus. The latter presented his researches just at a time when the world was prepared to accept anything in the shape of a small beast. Clinically it is not necessary to determine the presence of the gonococcus to establish the infective virulence of gonorrhœal pus, and this I believe is in accord with the many researches on the subject. Considering it from a pathological and surgical point of view, a clear history of recent impure sexual contact, in the presence of certain well-known clinical features, ignoring the presence or absence of gonococcus, will be accepted as sufficient evidence of the vice, and responsible for a large number of grave pelvic diseases, a much larger number than puerperal fever or syphilis is responsible for.

I think that I could easily prove my position by using the clinical evidence from my own college mates. I have taken pains to look them up and investigate their histories, that I might draw some conclusions in that way. I think that I can prove a causal relation in that way, having personal knowledge of their contamination.